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AAAI-14 Submission 1762

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Paper 1762

Title:	Generating Training Data for Learning Linear Composite Dispatching Rules for Scheduling
Paper:	PDF (Feb 05, 22:19 GMT)
Groups:	Heuristic Search and Optimization (HSO) Novel Machine Learning Algorithms (NMLA)
Author keywords:	Preference Learning Generating Training Data Trajectory Sampling Strategy Ranking Strategy Job Shop Scheduling Problem
EasyChair keyphrases:	pref pref pref (570), pref pref (246), training data (190), dispatching rule (126), job shop scheduling (95), mean relative error (79), ranking scheme (70), problem space (70), training set (70), preference set (70), percentage relative deviation (63), ranking strategy (60), linear ordinal regression model (60), processing time (60), linear composite dispatching rule (60), learning algorithm (50), suboptimal solution trajectory (47), generating training data (47), trajectory sampling strategy (47), most work remaining (47), partial schedule (40), statistical difference (40), supervised learning (40), suboptimal solution (40), optimal dispatch (40)
Topics:	HSO: Heuristic Search, HSO: Metareasoning and Metaheuristics, NMLA: Preferences/Ranking Learning
Abstract:	A supervised learning approach to generating composite linear priority dispatching rules for scheduling is studied. In particular we investigate a number of strategies for generating training data for learning a linear dispatching rule using preference learning. The results show that generating training data set from optimal solutions only is not as effective as when suboptimal solutions are added to the set. Furthermore, different strategies for creating preference pairs is investigated as well as suboptimal solution trajectories. The different strategies are investigated on 2000 randomly generated problem instances using two different problems generator settings.
Time:	Feb 03, 18:07 GMT

Authors

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Reviews

Review 2

1. Technical Quality:	3: (adequate, not a basis for accepting or rejecting the paper)
2. Experimental analysis:	4: (positive, a factor in accepting the paper)
3. Formal analysis:	4: (positive, a factor in accepting the paper)
4. Clarity/presentation:	4: (positive, a factor in accepting the paper)
QUALITY JUSTIFICATION:	<p>The paper is well written and well structured. Just note that it is difficult to well understand Figure 3 without any further explanation.</p> <p>The experimental section seems adequate.</p>
5. Novelty/innovation of question addressed:	3: (adequate, not a basis for accepting or rejecting the paper (or not applicable))
6. Novelty/innovation of application:	3: (adequate, not a basis for accepting or rejecting the paper (or not applicable))
7. Novelty/innovation with respect to aspects of human-level intelligence, complex cognition, or similar topics (Cognitive Systems):	3: (adequate, not a basis for accepting or rejecting the paper (or not applicable))
8. Novelty/innovation of solution proposed:	3: (adequate, not a basis for accepting or rejecting the paper (or not applicable))
NOVELTY/INNOVATION JUSTIFICATION:	<p>The paper presents several strategies for how to generate training data to be used in supervised learning of linear composite dispatching rules for jop-shop problems. A little remark about the novelty which seems to be barely enough for a full paper. In fact it seems that the paper is widely based on previous works. I therefore suggest the authors to show up and highlights the key ideas of the work.</p>
9. Breadth of interest to the AI community:	3: (adequate, not a basis for accepting or rejecting the paper (or not applicable))
10. Potential for impact to practical applications:	4: (positive, a factor in accepting the paper)
IMPACT JUSTIFICATION:	<p>I think that these studies have an interesting potential for impact to practical applications. I feel that this work is a good starting point for inviting novelties.</p> <p>The paper presents several strategies for how to generate training data to be used in supervised learning of linear composite dispatching rules for jop-shop problems.</p> <p>The paper is well written and well structured. Just note that Figure 3 is rather unclear. Please add further explanations.</p> <p>The experimental section seems adequate and the results are quite interesting.</p> <p>A little remark about the novelty which seems to be barely enough for a full paper. In fact it seems that the paper is widely based on previous works. I therefore suggest the authors to show up and highlights the key ideas of the work.</p>

SUMMARY OF REVIEW: I think that these studies have an interesting potential for impact to practical applications. I feel that this work is a good starting point for future (inviting) novelties.

To conclude I recommend this paper with a weak acceptance evaluation.

*** A FEW COMMENTS AFTER THE AUTHOR RESPONSE PERIOD ***

After reading the other reviews, I think that the lack of references to similar work in the Machine Learning community is a relevant obstacle to the paper acceptance. That said, I still think the idea is worth a deeper investigation: I suggest the authors to check the works mentioned by reviewer 1 and try a theoretical and/or practical comparison between such approach and the one they propose.

SUMMARY RATING: **-1:** (Weak rejection. No 1, 5 or 6 in any category, overall 3 or below.)

Review 3

1. Technical Quality:	2: (problematic, a factor in rejecting the paper)
2. Experimental analysis:	2: (problematic, a factor in rejecting the paper)
3. Formal analysis:	1: (flawed, a sufficient basis for rejecting the paper)
4. Clarity/presentation:	2: (problematic, a factor in rejecting the paper) The paper lacks clearance in presentation. Even though I see the intention of the authors to define different sampling strategies, the exact procedure remains unclear. A formal definition of how the sample are created is necessary. An experimental case study is provided.
QUALITY JUSTIFICATION:	3: (adequate, not a basis for accepting or rejecting the paper (or not applicable))
5. Novelty/innovation of question addressed:	3: (adequate, not a basis for accepting or rejecting the paper (or not applicable))
6. Novelty/innovation of application:	3: (adequate, not a basis for accepting or rejecting the paper (or not applicable))
7. Novelty/innovation with respect to aspects of human-level intelligence, complex cognition, or similar topics (Cognitive Systems):	3: (adequate, not a basis for accepting or rejecting the paper (or not applicable)) 2: (problematic, a factor in rejecting the paper)
8. Novelty/innovation of solution proposed:	2: (problematic, a factor in rejecting the paper)
NOVELTY/INNOVATION JUSTIFICATION:	I cannot see a particular innovation.
9. Breadth of interest to the AI community:	2: (problematic, a factor in rejecting the paper)
10. Potential for impact to practical applications:	2: (problematic, a factor in rejecting the paper) I believe that studying strategies to generate sample data is of significant interest to the community but this particular paper unfortunately does not provide a compelling insight into the problem.
IMPACT JUSTIFICATION:	The Problem of generating training/test

SUMMARY OF REVIEW:

The problem of generating training, test data is important but in this paper I cannot see novel approaches. The presentation lacks enough detail and formalization to fully comprehend the approach used and thus to interpret the results provided by the case study, in particular whether the rather abstract (artificial) setting the authors chose is reasonable.

SUMMARY RATING:

-1: (Weak rejection. No 1, 5 or 6 in any category, overall 3 or below.)

Review 1

- | | |
|---------------------------|---|
| 1. Technical Quality: | 3: (adequate, not a basis for accepting or rejecting the paper) |
| 2. Experimental analysis: | 4: (positive, a factor in accepting the paper) |
| 3. Formal analysis: | 3: (adequate, not a basis for accepting or rejecting the paper) |
| 4. Clarity/presentation: | 2: (problematic, a factor in rejecting the paper) |

QUALITY
JUSTIFICATION:

I should admit at the outset that I am reading this paper as something of an outsider -- I am not intimately familiar with the JSP or the surrounding literature which forms the context of this paper. So, it could simply be a function of that unfamiliarity, but I found this paper to be fairly difficult to read. I thought the presentation was organized fairly well, and, to the authors' credit, I do feel that I was ultimately able to reach a clear understanding of the question being asked and the approach to answering it. Nevertheless I did find myself struggling to uncover the main ideas of the paper, and especially by the end I had trouble keeping track of all the abbreviations and jargon. Ultimately, now that I do feel I have a good grasp on what the paper is after, I feel main ideas could have been laid out much more clearly and directly, and also at a higher level of abstraction. Though clearly the authors are focused on the JSP as the application domain, the question posed and the overall approach are not specific to the JSP, so they don't need to be mired in JSP-specific details. The clarity of the paper was not helped by the numerous grammatical errors throughout, though it was also not irrevocably harmed by them. I would nevertheless advise the authors to take another careful editing pass over the paper.

The centerpiece of the work is clearly the empirical study of the various methods of generating candidate schedules and preference rankings and their effect on the learnability of the scheduling heuristic. I felt that the experiments were thorough and addressed largely the right questions. The analysis of the results did help to illuminate why they came out why they did. A small complaint: the text on the figures was consistently too small to comfortably read. There was not really an attempt to theoretically/formally analyze the results, but that was appropriate as this is a paper focused on practice.

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|---|---|
| 5. Novelty/innovation of question addressed: | 1: (flawed, a sufficient basis to reject the paper) |
| 6. Novelty/innovation of application: | 3: (adequate, not a basis for accepting or rejecting the paper (or not applicable)) |
| 7. Novelty/innovation with respect to aspects of human-level intelligence, complex cognition, or similar topics | 3: (adequate, not a basis for accepting or rejecting the paper (or not applicable)) |

(Cognitive Systems):

8. Novelty/innovation of solution proposed:

2: (problematic, a factor in rejecting the paper)

Again, coming from an outsider perspective, I will not attempt to evaluate the novelty of this approach within the closely related context of the JSP literature. However, what I can offer is a deep connection to work that has been done in my research community. The good news is that I think someone has developed a well-principled, formal analysis of the core problem of this paper and provided a solution with strong theoretical guarantees. The bad news, I suppose, is that I feel this work needs to carefully explore that connection before it should be published so I cannot recommend it for acceptance.

NOVELTY/INNOVATION JUSTIFICATION:

The buzzword I would apply to this problem is "imitation learning" or "learning from demonstration." Briefly described at an abstract level this is the problem of a decision-making agent that has access to state/action trajectories performed by an expert (in this case, the generated schedules) and would like to learn to make decisions like the expert (perhaps by performing supervised learning with the state as input and the expert's decisions as output, as is done here). Ross, Gordon, and Bagnell (AISTATS 2014) pointed out the same problem encountered by the authors of this paper: that even an agent trained by a very good expert will often fail to perfectly reproduce the expert's behavior when it comes time to take action in the world. If the agent was trained only on expert trajectories it has only seen states reached by an expert, but when it makes a mistake in the world it reaches states the expert never would have, and doesn't know what to do, and often fails catastrophically at that point -- this is general principle underlying the authors' empirical observation that training on optimal trajectories is no better than training with random trajectories. RG&B solve this problem with an algorithm called DAgger that trains the agent on trajectories generated by its own learned policy, but with correcting labels provided by the expert. They prove that this approach, when combined with a no-regret learning algorithm, guarantees performance close to that of the best policy in the parameterized policy space (in this case the best possible weight vector).

All of which is to say that I believe the core problem investigated in this paper has already been carefully studied and a well-principled solution has been proposed. I don't see anything preventing DAgger from being applied in this setting. It is certainly possible that variations on DAgger would be best suited to the JSP or perhaps JSP-specific considerations would need to be brought to bear but I nevertheless think this study would need to be re-worked substantially in order to take into account the existing insights on this problem.

9. Breadth of interest to the AI community:

2: (problematic, a factor in rejecting the paper)

10. Potential for impact to practical applications:

3: (adequate, not a basis for accepting or rejecting the paper (or not applicable))

IMPACT JUSTIFICATION:

As mentioned above I felt the paper's tight focus on the JSP was perhaps detrimental to its potential impact. The problem being investigated is much deeper and more general than it is really presented here. Certainly there is a wide community rightfully interested specifically in scheduling problems, and they would likely find this paper interesting, but I feel the paper could be substantially stronger and have a wider reach if it looked up from the specific problem and gave more consideration to the general principles at play.

I think the authors have chosen an interesting question to answer

SUMMARY OF REVIEW:

SUMMARY RATING:

I think the authors have chosen an interesting question to answer and have developed some interesting insights. Unfortunately, I think they may have rediscovered some principles that have already been studied in a more formal and general setting. I also found the paper to be difficult to follow and possibly too focused on its specific application domain. Because of all this I do not recommend this paper be accepted.

-2: (Strong rejection. A 1 in some category, no 5 or 6 in any category.)